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AUTHOR Smith, John K.; Heshusius, Lous
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ABSTRACT

Educational researchers have claimed that the quantitative and qualitative approaches to educational inquiry are, indeed, compatible. However, it would be unfortunate to discontinue this debate. The quantitative-qualitative debate began with the interpretive approach to social inquiry. Dilthey argued that since cultural/moral sciences differ from natural/physical sciences, the context of the human experimenters' subjectivity, emotions, and values is significant. Concerned by inconsistent research interpretations, Weber unsuccessfully tried to synthesize a compromise between the two perspectives. Still, two distinct perspectives remained: the quantitative, realistic tradition describing independently existing social reality as it really is, and the qualitative, interpretive tradition assuming that social reality is mind-constructed according to internal coherence and social conditioning. More researchers considered the debate: Rist proposed a detente; Guba pursued criteria for qualitative or naturalistic inquiry. LeCompte and Goetz, Miles and Huberman, and Lynch de-emphasized the paradigmatic differences, implying that qualitative research is a procedural variation of quantitative inquiry. There are problems with this trend. Although the two approaches may be combined, each one's differing logic of justification affects the determination of validity. Other intellectual disciplines are also concerned with this debate. (GDC)

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CLOSING DOWN THE CONVERSATION: THE END OF THE QUANTITATIVE-QUALITATIVE DEBATE AMONG EDUCATIONAL INQUIRERS

John K. Smith
Department of Educational
Psychology and Foundations
College of Education
University of Northern Iowa
Cedar Falls, Iowa 50614

Lous Heshusius
Faculty of Education
York University
Ontario, Canada M3J 1P3

TM 850 350

CLOSING DOWN THE CONVERSATION: THE END OF THE QUANTITATIVE-QUALITATIVE DEBATE AMONG EDUCATIONAL INQUIRERS

A recent trend in the literature concerning the quantitative versus qualitative approaches to research indicates two things about the nature of this debate. First, many educational inquirers now accept that there are two different, equally legitimate, approaches to inquiry. Second, many inquirers also feel that whatever differences there may be between the two perspectives, they do not, in the final analysis, really make much of a difference. In other words, with the exception of the increasingly infrequent talk about conflicting paradigms or similar arguments about a fundamental difference in basic assumptions(see, for example, Guba, 1981,1983; Guba and Lincoln, 1981, 1982; Heshusius, 1982, and Smith, 1983a,1983b, and 1985), many educational inquirers now seem to think that the profession has reached a stage of, if not synthesis, then certainly compatibility and cooperation between the two approaches. The demand that an inquirer be "either-or" has been replaced by the injunction to employ both approaches in combination or to "draw on both styles at appropriate times and in appropriate amounts"(Cronbach, 1981, p. 223).

The contention of this paper is that this claim of compatibility, let alone one of synthesis, cannot be sustained--at least given our present state of thinking. Moreover, this unjustified "leap to compatibility" has had or will soon have the unfortunate effect of "closing down," without resolving, an important and interesting conversation. To defend and elaborate these points, this paper must address a number of topics. First, there is a discussion of the general outlines of the recent transition from conflict

to cooperation between the two perspectives. This review attempts to clarify the basis for the claim of compatibility. Then, an important but not well-heeded or necessarily well-understood distinction is made between method as technique and method as logic of justification. Confusion in this area has provoked confusion as to the differences between quantitative inquiry and qualitative inquiry. Finally, very brief speculation is undertaken as to the effects of "closing down" this conversation.

From Conflict to Detente to Cooperation

The contemporary history of the quantitative-qualitative debate can be traced back to the late nineteenth century and the development of, as a reaction to positivism, an interpretive approach to social inquiry(Hughes, 1958; Outhwaite, 1976, 1983; Smith, 1983a,1983b). From the perspective of educational inquiry, an overview of this period of concern over the legitimacy of and the relationship between the two approaches can be roughly divided into three phases. The first phase, begun by Dilthey and others(Benton, 1977) and set in place by Weber's failure to find a middle ground between their idealist orientation and the realist orientation of early positivism (Simey, 1969), lasted until recently. This phase was characterized by claims of fundamental differences in both assumptions and procedures and by an attitude that very often approached mutual disdain.

The second or transitional phase, for which the work of Guba(1978, 1981), Guba and Lincoln(1981,1982), and Rist(1977) are particularly important, was characterized by an acceptance of, but a decreased concern over, paradigmatic differences, a much increased emphasis on procedural issues, and by an attitude of detente. This stage marks the beginning of an intensive effort to develop criteria and procedures for qualitative inquiry com-

parable to(in that they will allow for similar claims of validity and reliability) those of quantitative inquiry. In the current phase the concern over assumptions is minimal(either they are no longer felt important and/or there is an implicit acceptance that both perspectives are grounded on the same assumptions), differences are confined primarily to the area of techniques, and the attitude is one of compatibility and active cooperation.

Conflict

Dilthey's elaboration of an interpretive approach to what he called the "cultural or moral sciences"(Hodges, 1944, 1958; Hughes, 1958; Ermarth, 1978) offered a direct challenge to positivism. He argued that there was a fundamental difference in subject matter between the natural and social areas which, given that positivism was based on a natural science model, made it unacceptable as a means of social inquiry. Whereas the physical sciences dealt with a series of inanimate objects that could be seen as existing outside of us(a world of external, objectively knowable facts), the moral sciences focused on the products of the human mind as these products were intimately connected to human minds with all their subjectivity, emotions, and values. From this he concluded that social reality was the result of conscious human intention and that the interrelationships of what was being investigated and the investigator were impossible to separate. For all people, lay people and social inquirers alike, what actually "existed" was what they thought existed. There was no objective reality as such that was divorced from the people who participated in and interpreted that reality(Bergner, 1981, p. 64).

Based on this point(and others of course) Dilthey said that the study of social life must involve the ideas of lived experience and interpretive understanding. He said that there were two ways to experience: inner-lived

experience and sensory experience. The latter pertained to the physical sciences, whereas the former, which referred to the imaginative recreation or identification of one's own mental life with that of others, was crucial to the human studies. In other words, one's knowledge of the mental world and its objectifications was based on the lived experience one shares as a part of that mental world. This meant that the investigator could only attain an understanding of another through a process of interpretation--one that inevitably involved a hermeneutical method. The meaning of human expression was context-bound and could not be divorced from context. To understand an expression one must understand the context and to understand the context one must understand the individual expressions. Hermeneutics required a constant movement of interpretation between parts and whole--a process that can have no absolute beginning point or ending point.

Weber was impressed with Dilthey's position that the human studies must be based on the ideas of context, meaning, understanding, and so on. However, he did not accept the physical-social subject matter split and, moreover, he recognized, as did Dilthey, that the latter's ideas led to a major problem. That is, if meaning must be taken within context(both that of the action or utterance itself and that of the interpreter) and understanding must be hermeneutical, than the interpretation of expressions could vary. Given this prospect, Could there be any such thing as a correct interpretation? In contrast to developments on the positivist side, Dilthey could find no criteria, short of an unacceptable(to him) appeal to metaphysics, to use in sorting-out conflicting possibilities(Hughes, 1958, p. 199).

Weber's solution to this problem was to attempt to bring together the two perspectives(Aron, 1967,1970; Benton, 1977; Outhwaite, 1976, 1983; Simey,

1969). He felt that the realist-oriented tenets of positivism were wanting in that they could not deal adequately with that particularly human aspect of human existence--the ability to act intentionally and to ascribe meaning. Likewise, he felt that the idealist-oriented approach lacked a sufficient understanding and respect for social reality as an existing reality. Weber therefore attempted to achieve a middle-ground between, in the sense of a synthesis of, the two perspectives. A good case can be made that in the end he was unable to reach this goal.(Simey, 1969). This failure left the two perspectives openly and definitively apart. That inquirers from both approaches continue to directly or indirectly draw on his work in this area reflects this failure of synthesis.

By shortly after the turn of this century, two distinct perspectives on social inquiry were in competition. These possibilities differed, and have continued to differ, in terms of their basic assumptions. The quantitative tradition, given its realist orientation, is based on the idea of an independently existing social reality susceptible to being described as it really is. Truth is defined or characterized as a correspondence between our words and that independently existing reality. The point of view common to this perspective, which allows that facts can be held separate from values, is what Putnam(1981) calls a "God's Eye" point of view(p. 49).

The qualitative or interpretive tradition, based on an idealist temperament, takes the position that social reality is mind-dependent in the sense of mind-constructed. Truth is, at one level, a matter of internal coherence of our statements and, at a second level, a matter of socially and historically conditioned agreement. Finally, social inquiry cannot be value-free and we cannot adopt a "God's Eye" point of view--all we can have are the various points of view of various people based on their particular interests, purposes, and

values(Putnam, 1981, p. 50).

While it is of course impossible to characterize the attitude of every social researcher since Weber, it is not difficult to demonstrate that in general the relationship between the advocates of each perspective was an uneasy one during this period. Outhwaite(1983) describes this era as one of an "endemic opposition between conflicting frameworks"(p. 2). Moreover, this endemic opposition was often punctuated with comments that reflected an attitude of, if not disdain, certainly something close to it. Louch(1969), for example, virtually mocked the empiricist enterprise when he labeled sociologists and anthropologists as tellers of tales of no scientific import(p. 160). Rudner(1966), on the other side, has referred to many arguments on the qualitative side with phrases such as "egregious confusion," "patently wrong-headed rational," and so on(pp. 69-70).

Very much the same situation prevailed in the area of educational inquiry. Rist(1977) has cogently described the opposition during this first phase between advocates of the two perspectives. He felt a great deal of the discussion had been characterized by "trite cliches," had been dominated by the idea of "simple and rigid polarities," and there was a "continual fixation upon what is 'good' about one approach or 'bad' about another"(p. 42).

Detente

After noting this period of disdain, Rist went on to state that a time of detente had begun. Even though the two approaches have different epistemological traditions and tensions remained, "a set of accomodations is emerging whereby the various approaches . . . are recognizing the right of 'peaceful coexistence'"(p. 42). However, since "we suffer from the lack of appropriate language and conceptual frameworks for locating both paradigms in relation to one another"(p. 48), he specifically held that this easing of tension did

not mean that synthesis would be forthcoming. Anything more than detente may not be possible--or, if it is, it would require considerable time and much serious analysis. Looking back over the last few years, it is clear the movement developed in ways that Rist did not anticipate. What he felt would be a long period of detente turned out to be a short-lived and basically transitional phase on the road to the claim of compatibility and the call for cooperation. A brief examination of the work of Guba(individually and with Lincoln), even though it was not intended to provoke or aid this transition, will show how this situation came about.

In various places Guba(see, in particular, 1978, 1981, and Guba and Lincoln, 1981,1982) was one of the first to seriously pursue criteria and procedures for qualtiative(or naturalistic as he calls it) inquiry. These discussions clearly indicate that he accepts that the two approaches differ at the paradigmatic level. He discussed the opposing assumptions within much the same categories, with different terminology in some places, as were employed above. Rationalistic inquiry is based on the ideas of a single, independently existing reality, there is an independence of the inquirer from what is studied, facts are separate from values, and so on. Naturalistic inquiry responds to the ideas of multiple, mind-dependent realities, of the interdependence of inquirer and subject, chat inquiry is value-constrained, and so on.

One thing must be immediately noted about this discussion. Even though Guba feels that the rationalist side is in error and naturalism is more sound, the tone of his disagreement with the former paradigm is quite moderate. Guba evidences little interest in provoking or continuing a major battle at this level. Accordingly, while he noted that the rationalist approach has not produced outstanding results, that its axioms are only poorly fulfilled

in the area of social inquiry, and so on, these points are not phrased in sharp language with a demand to abandon rationalism in favor of naturalism (Guba and Lincoln, 1982, pp. 234-236). In general, while he obviously finds naturalism superior, the spirit is still one of detente rather than one of provoking conflict.*

At another level there is an aspect of Guba's work which, while not intended to have this result, assisted others in making their claims for compatibility and cooperation. The point is that his frame of reference for the elaboration of criteria and procedures is not only the assumptions he posited for naturalistic inquiry, but also the criteria and procedures that characterize rationalist inquiry. In another paper(1985) I argue that there is a distinct tension is his discussion of criteria and procedures. This tension results from an uneasy balance between his acceptance of epistemologically antifoundationalist assumptions and his desire to develop foundational rules to sort out the good from the not-so-good qualitative study(the desire for certitude).** The problem is that these two elements are incompatible. At any rate, the combined reference point for developing criteria and procedures allows that many of his injunctions can be interpreted as more "at home" with the rationalist paradigm than with the naturalist one. Despite

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*There are other places where Guba is more agressive in his criticisms of the rationalist paradigm and less accepting of detente. In a recent paper(1983), for example, he strongly criticizes the idea of compromise, bluntly states the situation is one of 'either-or'(p. 3), and ends with a call for confrontation(p. 30).

**In one article(1981) Guba, in places, disclaims the intention to be foundationalist. He says that his criteria are not prescriptions and he does not want an 'orthodoxy'(p. 90). However, there is considerable evidence that he desires his criteria to be more than simply characterizing traits. Thus, he refers to these criteria as rules(p. 90), as safeguards(p. 76), and says that they are to be applied whenever we judge naturalistic inquiry(p. 88). This is all rather strongly put and leads to the conclusion that he would like, antifoundationalist assumptions notwithstanding, to be foundational.

his acceptance of a fundamental difference in assumptions, in the end his discussion of criteria and procedures has the effect of blurring these differences.

Because of time and space limitations, only a brief examination can be undertaken of the structure and content of this aspect of his work. Guba's development of criteria and procedures for naturalistic inquiry seems partly motivated by the idea 'if this is how the quantitative approach handles a particular problem, this is how the qualitative perspective can handle that problem'. This results in a discussion structured with close reference to the "comparable" criteria and procedures of the rationalist side--almost a parallelism. This parallelism is evident in his discussion of the elements of trustworthiness of naturalistic inquiry as compared to those of rationalistic inquiry; credibility is matched against internal validity, dependability is referenced to reliability, confirmability to objectivity, and so on(1981, p. 80). While this parallelism is not nearly as complete and direct as it later is for other authors, it is sufficient to direct attention away from basic philosophical oppositions. That is, one can easily get the impression that the two approaches are variations in techniques within the same assumptive framework, to reach the same goals, and much less a matter of paradigmatic conflict.

An analysis of his discussion of criteria and procedures demonstrates how this impression can be obtained. In the example of truth value, Guba said that for the rationalist approach it is a question of internal validity which in turn is a question of isomorphism between the data(or the inquirers statements) and an independently existing reality that the data reflect(pp. 79-82). This is in effect to define truth in correspondence terms. For naturalism the problem is one of credibility, which also employs the idea of

isomorphism. However, in this case the issue is one of isomorphism to the perceptions or interpretations a person gives to a situation--an interpretation which, given the idea that reality is mind-constructed, becomes reality as it is for that person at any given time and place. Thus rationalism can achieve truth value to the extent that an inquirer's statements correspond to how things really are, whereas naturalism can achieve truth value to the extent an inquirer's statements correspond to how people out there really interpret or construct their realities.

In the final analysis internal validity and credibility begin to look like the same thing. To fulfill the injunction in either case requires the assumption that what is known--be it an existent reality or an interpretive reality--stands independent of the inquirer and can be described undistorted by the inquirer's interests, values, and purposes. In other words, isomorphism makes sense as a criterion only if one accepts the rationalist position on the separation of mind and world with the associated ideas about neutrality and objectivity(that God's Eye point of view). Naturalistic assumptions, such as reality as mind-constructed and that facts cannot be separate from values, undermine the possibility of isomorphism and the ideas of neutrality and objectivity as defined from a rationslist perspective. Naturalism leads to the prospect that all an inquirer can offer is an interpretation(based on his/her interests and purposes) of the interpretation of others(based on their interests and purposes). Recalling Dilthey, this process is epistemologically antifoundational in the sense that it can have no definitive beginning or ending points. At any rate, Guba's discussion of credibility allows this criterion to look very much like internal validity and, as such, it is more 'at home' with the assumptions of rationalism tham those of naturalism.

The same conclusions can be drawn for the other criteria Guba discusses such as applicability(external validity-transferability), consistency (reliability-dependability), and neutrality(objectivity-confirmability). In each case an analysis of the latter concept will again reveal that it is very close to a restatement, in different terminology, of the former concept (pp. 79-82). Hence, Guba's serious and detailed efforts to develop different criteria and procedures for qualitative inquiry, based on a different paradigm, has had the unintended effect of blurring philosophical distinctions and left the impression, again unintended, that the differences between the two perspectives are primarily those of technique.

Compatibility and Cooperation

The last phase has taken Guba one step further in all three areas. Whereas Guba held to fundamental differences at the paradigmatic level, the present tendency is to either ignore such differences or, if they are noted, to "de-epistemologize" the situation by enjoining that one need not be particularly concerned by these issues. For many authors, the most pressing problem is to develop criteria and procedures for qualitative inquiry that have the same import as the criteria and procedures of quantitative inquiry(act as a constraint on our subjective selves, allow for the possibility of certitude, and so on). This concern has pushed the parallelism that Guba "flirted" with to become, in some cases, quite complete and direct. The major consequence of this combination of little interest in assumptions and a strong emphasis on how-to-do-it questions is that many inquirers now conceptualize the quantitative-qualitative debate as a discussion of variations in techniques within the same logic of justification. Inquiry and its results are subject to the same interpretation in both cases--only aspects of practice differ. Given these developments, it is not surprising that detente has given

way to the claim of compatibility and the call for cooperation.

LeCompte and Goetz(1982) provide an excellent example of the situation in which differences in philosophical assumptions, and the consequences for inquiry of such differences, are for the most part ignored. In place of this topic, they discuss various techniques that qualitative inquirers may employ to make their approach equally as rigorous and systematic as quantitative inquiry. The discussion is very much structured in a parallel fashion--if this is how a certain criterion can be met for quantitative inquiry, then this is how the same criterion can be achieved for qualitative inquiry. The end result of this approach is that they feel, or certainly leave the impression that they do, the two approaches are essentially the same except for differences at the level of practice.

LeCompte and Goetz begin by noting a series of contrasts between the two perspectives. Their discussion here is structured around experimentation versus ethnography(pp. 33-34). By putting the issue in this way, they are led to discuss factors such as that of researcher involvement with what is investigated(manipulation versus observation in natural settings), of when theory enters the process(prior to data versus emerges from data), and so on. While these differences are interesting and could possibily lead to various distinctions of epistemological and ontological significance, LeCompte and Goetz do not go in this direction. Rather, not only do they by-pass an examination of paradigm differences and the implications thereof, they in effect collapse such differences and obscure consequences by defining certain crucial elements in the same way for both perspectives.

The basic concepts of validity and reliability and their various subdivisions are interpreted no differently for qualitative inquiry than they are for quantitative inquiry. If validity is a matter of accuracy of repre-

sentation of empirical reality for quantitative inquiry, then it is such for qualitative inquiry; if reliability is a matter of replication for the former, then the same standard is to be applied to the latter(p. 32). This is all well and good with the exception of the fact that to make sense of these concepts, when defined in this way, requires that one accept realist assumptions such as the separation of investigator-investigated, that truth or credibility should be characterized in correspondence terms, and so on.

Their discussion of validity can be used to illustrate this point. The standard quantitatively oriented definition of validity, which they accept, is based on the prospect of matching "explanations of the world with the actual conditions in it"(p. 43). In other words, validity is attained when our statements accurately reflect how things really are out there. This definition, if it is to have any force at all, must entail the assumptions that reality exists independent of us and can be known as it really is independent of our interests, purposes, and values. In effect, this is to state validity, and thereby the credibility of research results, in correspondence terms.

If reality is mind-constructed or mind-dependent in the sense that no description of the world can be offered apart from the describer's interests and purposes(Putnam, 1981, p. 50), the situation looks very different. This assumption, central to idealist-oriented approaches to inquiry, undermines the standard definition of validity. If nothing else, it disallows exactly what is need to determine whether or not our words correspond to actual conditions--the possibility of independent access to both our minds and an independently existing, uninterpreted, reality(on this point see, among many others, Goodman, 1978; Putnam, 1981, Rorty, 1979). Since we cannot have such access and hence cannot compare our descriptions to actual

conditions, this assumption leads to the conclusion that all we can do is match descriptions with other descriptions or conceptualizations--a process which has no definitive ending point. Validity in this sense may be more appropriately defined as an "honorific" applied to an explanation, from among other explanations, with which one agrees. This is a choice that is ultimately based on the interests, purposes, and values of the chooser.

If the two approaches do not differ at the paradigm level, then in what ways can they be distinguished? For Leompte and Goetz the differences are consigned to the area of the techniques each side employs to fulfill the criteria of validity and reliability. For example, quantitative inquiry attempts to handle the problem of testing and instrumentation effects through the development and proper application of standardized instruments. The parallel problem of observer effects for qualitative inquiry can be alleviated by, among other things, an investigator's constant check and recheck of the meanings of his/her interpretations with the subjects(pp. 37-40). Or, in a more general example, there is the issue of accumulated detail versus established procedures. That is, what quantitative inquiry is able to obtain through the use of instrumentation, sampling procedures, and so on, qualitative inquiry achieves through the technique of amassing detail(thick description) based on extended engagement in the field. Contrary to Guba's attempt to develop criteria and procedures with one eye on a different set of assumptions and the other on the techniques of quantitative inquiry, the referent point for LeCompte and Goetz is almost solely the latter. And, by ignoring paradigm differences and adopting this parallelism, they make qualitative inquiry little more than a procedural variation of, within the same conceptual framework as, quantitative inquiry.

Miles and Huberman(1984) take a different approach to the question of

paradigmatic differences than do LeCompte and Goetz. Whereas the latter basically ignore such issues in pursuit of technique differences, the former recognize philosophical assumptions as important, but then in a sense "de-epistemologize" the debate as they move on to their principal concern of how to do qualitative inquiry. In the end, even though Miles and Huberman are less obvious, and hence more difficult to interpret, in this regard, they also leave the impression that qualitative inquiry need not be seen as much more than a procedural variation on the quantitative theme.

Miles and Huberman "de-epistemologize" the debate by noting that even though epistemological issues constitute more than a "nontrivial battle"(p. 21), it is not one with which researchers need be preoccupied--or even necessarily occupied. Among the reasons for this injunction are that the epistemological debate will not be resolved in the near future, researchers already "blend" the two perspectives, and such paradigmatic problems divert attention from the critical aspect of developing "a body of clearly-defined methods for drawing valid meaning from qualitative data"(p. 21). Thus, as far as philosophical questions go, they recommend that researchers be epistemologically ecumenical and leave the larger debate to those who are most interested in it. This position leaves them free to label themselves as "middle-range epistemologists," "soft-nosed positivists," and "right-wing qualitative inquirers" and then proceed to various techniques that will make qualitative inquiry "scientific in the positivist sense of the word"(p. 21).

All of this has a certain appeal in that it appears to "free-up" researchers from some nagging questions as to how they may interpret the process of inquiry and its results and thereby allows researchers to "get down to business." The concern of Miles and Huberman that "epistemological purity doesn't get research done"(p. 21)expresses this point. Moreover,

additional comfort can be drawn from the reaction, or lack of reaction, of practicing natural scientists to Kuhn's work. As is commonly acknowledged, Kuhn's ideas had little or no impact at this level--only philosophers were upset. However, there are some problems here. Empiricist social and educational inquiry has not achieved, and probably never will achieve, the same intellectual and practical mastery of subject matter as has been the case for natural science. If it had in fact, one could easily argue that the qualitative-quantitative debate would not have reached the proportions it has. At any rate, since social inquiry, unlike natural science, cannot stand behind what are generally considered obvious or self-evident accomplishments, it is not clear what the business is that one is left to "get down to" by becoming epistemologically ecumenical.

The crucial thing about dismissing paradigmatic questions in this fashion is that it leaves many concepts defined in the same way for both perspectives. While some ambiguity exists in this regard, Miles and Huberman do seem to accept that concepts such as valid, real, dependable, and trustworthy can be undertaken in the same way for both sides. The ambiguity arises because they employ terms such as truth space(p. 22), wholly contradictory findings(p. 27), and reasonably communicable sets of procedures(p.22). This situation, however, is best seen as advocating a "loosening" of the constraints, the avoidance of a highly "mechanical orthodoxy," it does not constitute a recognition that a different paradigm requires different interpretations of these concepts and hence of inquiry and its results.

That Miles and Huberman allow the differences between the two approaches to fall primarily in the area of techniques can be further noted in their discussion of method. Even though they do not adopt a strict parallelism, there is a good deal of "mirroring"(Marshall, 1984, p. 26) of quantitative

techniques--especially in the sense of what method is to accomplish for qualitative inquiry. While their arguments are somewhat qualified, it is still clear that established methods will do for the latter what they have supposedly done for the former: They will prevent self-delusion(p. 21), prevent inquirers from falling prey to bias and deception(p. 27), and serve as a basis for judging the reasonableness of conclusions(p. 22). Moreover, it is precisely such a set of valid methods that will allow for the discovery of lawful relationships in the social world--relationships, given the way they use the term, that they hold exist independent of researchers and that can be known as they really are. Thus, a certain "looseness" notwithstanding, epistemological ecumenicalism and methods that will make qualitative inquiry "scientific in the positivist sense of the word"(p. 21), makes this approach little more than a variation on the quantitative theme. As it was for LeCompte and Goetz, Miles and Huberman transform the paradigmatic debate into a discussion of methodological variations within a realist framework.

Finally, one other article must be noted in regard to this third phase of the debate. Lynch(1983) has argued that there really is no debate in that the only points of difference for all inquiry involves the degree of manipulation of setting and the degree of structure in the units of measurement. Inquiry that manipulates the setting and subjects is at one end of a continuum with observation in natural settings at the other. Likewise, anecdotal or open-ended data is at one pole and structured response at the other. When placed in a two-by-two matrix, this leads to structured data in natural settings, unstructured data under experimental conditions, and so on. Lynch has reduced the issues to simply ones of researcher"posture" in regard to setting, subjects, and data. The original paradigmatic debate is hence little more than a discussion of the traditional distinction(within the

quantitative perspective) of descriptive versus experimental inquiry. This conceptualization of the issue is about as far away as one can get from Dilthey's hermeneutic challenge to positivism and from Weber's monumental attempt to find a middle ground between the two perspectives.

Summary

What began as a significant debate between two different approaches to inquiry based on different philosophical assumptions, has become an "in-house" discussion of no pressing interest or consequence. That there are, or even might be, paradigmatic differences that require different interpretations of inquiry and its results, is no longer taken seriously. At present, the principal concern is to develop methods for qualitative inquiry that will allow this approach to claim certitude, as is the case for quantitative inquiry, for its findings. This combination of avoiding assumptions and the parallel development of methods has transformed, inadverently and implicitly, qualtiative inquiry into a procedural variation of quantitative inquiry. The former perspective has been "captured" by the latter perspective in the sense that both have come to share the same realist-oriented assumptions. Given this situtation, the claim of compatibility and the call for cooperation are not surprising--as presently conceptualized there are no differences between the two perspectives that make any difference.

Misconceptualizations about the Issue

The question now arises as to what lies behind this rapid transformation of a paradigmatic debate into a discussion of variations in techniques. Two factors are important: 1) a confusion over the definition of method and the relationship between possible definitions and 2) an uncritical dependence

on the idea that inquiry is a matter of "what works." Both claims require elaboration--the first at some length and the second more briefly.

Method can be defined or characterized in at least two ways. The standard, most commonly encountered, meaning is method as procedures or techniques. In this sense the term invokes the kinds of "how-to-do-it" questions found in introductory textbooks for both quantitative and qualitative inquiry. From the former perspective the focus is on issues of how to do various statistical procedures, sample, construct measuring instruments, and so on. From the latter perspective, the discussion involves techniques such as how to engage in participant observation, analyze field notes, and perform member checks. Moreover, an interest in method at this level can also be expressed, as it was directly by LeCompte and Goetz(1982) and less directly by Miles and Huberman(1984), in terms of a comparative or parallel examination of how each side goes about various aspects of inquiry.

The second characterization of method is "logic of justification." Here, in a sense more common to European social philosophy, the focus is not on techniques but on the elaboration of logical issues and, ultimately, on the justifications given in support of practice. When employed in this fashion the term takes on the meaning given it by various people such as Durkheim (1938), Weber(1949), and more recently, Kaplan(1964), and Giddens(1976). This conceptualization of method inevitably involves very basic epistemological and ontological questions on the order of, What is the nature of social reality? What is the relationship of the investigator to investigated? and How is truth to be characterized or defined?

Given these different characterizations of method, the important issue concerns how the relationship between them is conceptualized: What does logic of justification have to say about technique and vice versa? To examine this

question one can begin by noting that at the level of the logic of justification, involving as it does basic philosophical assumptions, are differences of major consequence between the two perspectives. Even Miles and Huberman(1984), their dismissal of epistemological issues notwithstanding, note that this constitutes more than a trivial battle(p. 20). At the level of technique are relatively uninteresting differences between the two perspectives. Questions in this case are narrowly based ones of, Can qualitative researchers supplement naturalistic observation with the quantification of events? or, Can quantitative inquirers supplement their controlled instrumentation with open-ended observation in natural settings?

The crucial point is that the answers to these types of questions is quite simply yes. The logic of justification does not impose a set of detailed boundaries that directs or determines every judgment about practice. Researchers who accept an idealist-oriented logic of justification are not prohibited from the use of certain techniques normally associated with quantitative inquiry. The same, of course, applies in reverse. While inquirers on each side may not be interested in or see the need to "borrow" a particular technique, no good argument can be made that they are logically prevented from doing so. Thus, one may grant that various authors such as Cronbach(1983), Miles and Huberman(1984), and Reichardt and Cook(1979) are quite correct in their claim that individual techniques can be "mixed."

However, to allow that techniques can be "mixed" at this individual level cannot lead to the conclusion, which has been at least implicitly taken by many of these same authors, that the two perspectives are compatible or complementary. The defence of this claim requires an examination of one aspect of inquiry. Certain specified and coherently arranged sets of techniques, as opposed to particular, individual practices, are thought necessary to estab-

lish major conditions of inquiry such as the validity of measuring instruments, the internal validity of studies, and the reliability of research findings. Since these major conditions depend on the proper application of ordered practices, they can be thought of as "linkage" points between method as logic of justification and method as how-to-do-it. The crucial issue here is that how one characterizes these conditions depends not on the techniques employed, but rather on the logic of justification one accepts. That is, the meaning assigned to the term valid, as in the compliment, "this study is internally valid," is taken not from the practices involved but rather from how truth is defined. The epistemological position constrains how the condition is conceptualized and, by extension, it directs the particular set of techniques that must be performed(that is, given certain assumptions, if there are any techniques that can be so priviledged) to achieve that condition.

Given this point, it is then clear that if the two perspectives define truth differently, not only must each accept a different conceptualization of validity, but each must also hold to a different interpretation of the relationship of procedures to the claim of validity. For the quantitative perspective, in that truth is defined as correspondence, the label valid announces results that reflect or correspond to how things really are out there is the world. These results constitute the discovery of relationships whose existence is independent of a researcher's interest in, feelings about, or evaluations of, them. Moreover, a judgment of validity in this case is very much based on a judgment that proper methods or sets of techniques were employed. In fact, proper procedures, properly applied, lead to results that are thought to be compelling--so much so that to not accept such results may provoke the criticism that one is being irrational or stubbornly subjective. For quantitative inquiry, therefore, certain specific sets of techniques are

considered epistemologically priviledged in the sense that their correct application is necessary to achieve validity or to discover how things really are.

One feature of the conventional approach to test validity provides a further example of this situation. Test validity is commonly divided into cognitive and normative parts(Messick, 1980). The first part involves the scientific-technical assessment of the facts or evidence. The issue here is whether or not a test accurately measures what it is supposed to measure. The second part focuses on ethical questions regarding the social consequences of employing a particular test. For our purposes, the crucial difference between the two parts is that procedural considerations are vitally important to judgments on the cognitive side, but unimportant for the normative aspect. Since "empirical" validity is obviously an evidential matter it must be based on evidence that has been, necessarily so, properly gathered and analyzed. Dissent as to the validity of a test will, because of this necessity, in large measure focus on what was done and how it was done. If the process was correct on both counts, the burden then falls on the dissenter to either accept, because of the evidence, the claim of validity or run the risk of being labeled unscientific or irrational. Dissent over normative judgments cannot, of course, be resolved by assessing the procedures employed to develop the judgment. In other words, no normative argument can be granted the similar priviledge of being compelling simply because it was developed in a certain way. Thus, in the first instance there is, so to speak, a "bottom line" or a foundation to stand on--one that is available to us through the correct application of the correct procedures. In the second instance no such foundationalist claims are possible and, accordingly, no procedures can have a priviledged status(see Smith, in press, for a detailed

discussion of this example of test validity).

From the perspective of qualitative inquiry this line of reasoning is impossible to bring into focus. The assumptions in this case, in contrast to those supporting quantitative inquiry, do not pose a foundational epistemology and, by extension, allow that certain sets of procedures are epistemologically priviledged. The idealist oriented assumptions of reality as mind-constituted, no separation of facts and values, truth as agreement, and so on are anti-foundational in that they undermine the prospect of independent access to an independently existing reality. It is the absence of this possibility that leads to the idea that a description can only be matched to other descriptions and not to an unconceptualized reality. This reawakens the Dilthian issue noted earlier--if meaning must be taken within context(that of the subjects, the investigator, those who read the investigation, and so on) and the process is hermeneutical, on what basis does one choose from among descriptions. In other words, if all we have are various interpretations of reality of various people based on their various interests, purposes, and values, What meaning must be given to valid and how does one judge an interpretation valid or invalid?

Within the qualitative paradigm, valid is best defined as an "honorific" applied to an interpretation with which one agrees. The ultimate basis for such agreement is that the interpreters share, or come to share, based ideally on open dialogue and justification, similar values and interests. As Taylor (1971) puts it,"Ultimately, a good explanation is one which makes sense of the behavior; but then to appreciate a good explanation, one has to agree on what makes good sense; what makes good sense is a functions of one's readings; and these in turn are based on the kind of sense one understands"(p. 14). There is a circularity to this interpretive process(Dilthey's point that the

hermeneutical process had no definite beginning or ending points) that cannot be broken out of--even by methodological prescriptions. Whereas the foundationalist assumptions of quantitative inquiry allow that proper techniques will prevent this circularity(lead to certitude), such is not the case for qualitative inquiry. The antifoundational assumptions mean that technique depends on the context and on what it makes sense to do in any particular context--but, of course, what makes sense in any particular situation depends on the kind of sense one understands and so on. One may be interested in how a researcher did a study and may agree with the techniques employed or argue that other should have been used, but this is quite different from the claim that certain procedures are necessary. Qualitative inquiry finds it impossible to hold that certain things must be done or that validity is a matter of proper techniques properly applied.

What can be said of this situation for the claim of compatibility? Quite simply, a confusion of method as logic of justification with method as technique has allowed many people to make, even if implicitly, an erroneous conclusion. That individual techniques can be mixed does not mean there are no paradigmatic differences of concern. If one begins with the different assumptions and traces out their implications it is clear that the two perspectives part company over various issues such as the role of techniques, the conceptualizations given basic conditions such as validity and reliability, and the interpretation of research results. Quantitative inquiry pretends to certitude; to the idea that our descriptions can match actual conditions in the world and that we can know when this matching occurs and when it does not. This certitude is available primarily through an adherence to proper procedures. Qualitative inquiry finds all of this unacceptable. In this case, inquiry is a never-ending process(hermeneutical) of interpreting the inter-

pretations of others. All that can be done is to match descriptions to other descriptions--choosing to honor some as valid because they "make sense" given one's interests and purposes. What eluded Weber still eludes us and no declaration of compatibility or that we are beyond the issue will achieve what he failed to achieve.

Finally, a brief comment must be made in regard to how the claim of compatibility is supported by the idea that research is a matter of "what works." Even though expressed in different terms, this idea is present in many discussions of quantitative-qualitative inquiry. For example, this is in essence what Miles and Huberman mean when they say that "epistemological purity doesn't get research done"(p. 21) and what Reichardt and Cook mean with the comment that one should mix the approaches in order to "satisfy the demands of evaluation research in the most efficacious manner possible"(p. 27). While this idea is appealing at one level, in that it calls up the image of the educational researcher using whatever is necessary to get the job done and solve serious educational problems, there is a problem here.

The problem is that "what works", no matter how expressed, is really little more than a formal statement that tells us nothing about the process of inquiry and the interpretation of its results. Putnam's(1981) discussion of the phrase "science seeks the truth" will, by analogy, illustrate this point. In this case, Putnam says that for people to say that science seeks the truth is an empty statement in the absence of knowing "what they consider a rational way to pursue inquiry, what their standards of objectivity are, when they consider it rational to terminate an inquiry, [and] what grounds they will regard as providing good reason for accepting one verdict or another on whatever sort of questions they may be interested in"(p. 129). In other words, "truth is not the bottom line(Putnam's emphasis, p. 130) because it

receives its standing from the goals one accepts for inquiry, the criteria to be applied, and so on.

Much the same analysis can be given to the idea that research decisions can be taken on the basis of "what works." This idea, as with truth, is not the bottom line in that it depends on the goals one holds for inquiry, the criteria employed for judgments, and so on. If one holds that inquiry is a matter of matching statements to actual conditions, "what works" will be very different than if one finds inquiry to be interpretations of the interpretations of others. One response to this point must be noted. One may argue that this problem does not arise because all inquiry is based on criteria such as fruitfulness, simplicity, accuracy, and scope. However, these criteria do not solve the problem because, as Kuhn(1977) noted, these terms are value terms and, accordingly, they may be interpreted differently depending on the situation or context. As he said, "individually the criteria are imprecise: individuals may legitimately differ about their application to concrete cases"(p.322). In the end, "what works" is not a firm foundation to stand on--what works depends on the kind of work one wants inquiry to do, which, in turn, depends on the paradigm within which one is working.

Summary and Implications

Over the last few years the quantitative-qualitative issue, at least as conceptualized by many educational researchers, has gone from a state of conflict to one of compatibility and cooperation. The contention of this paper has been that this transformation cannot be legitimately sustained in that it is based on the inadverent movement of qualitative inquiry into the quantitative camp. That is, the idea of compatibility is primarily supported by the misconception, implicit if not explicit, that both perspectives share

the same basic realist-oriented assumptions and, hence, qualitative inquiry is in essence little more than a procedural variation of quantitative inquiry. This paper also attempted to demonstrate that this transformation has been aided by a confusion over the relationship between two different definitions of method--as logic of justification and as technique--and by the unfounded idea that "what works" can serve as the bottom line for decisions about research. At any rate, it is clear that many educational researchers have recently taken the unacceptable and unfortunate conclusion that there are no differences between the two perspectives that really make any difference.

Finally, a brief word as to why this "closing down of the conversation" must carry the label unfortunate. In general terms, it is unfortunate that educational researchers have moved away from this debate at a time when it is of major concern across a broad intellectual horizon. It can be quite easily demonstrated that the issues involved in the quantitative-qualitative debate, even though expressed in different ways, are of major concern in areas such as history(Stone, 1979), physics(Wheeler, 1975), anthropology(Geertz, 1980), linguistics(Lakoff and Johnson, 1980) and, of course, philosophy-philosophy of science(among many others, Bernstein, 1983; Goodman, 1978; and Rorty, 1979). Moreover, various novelists such as Durrell(1958-1960) and the contemporary Latin American writers such as Donoso(1984) and Garcia Marquez(1970, 1975) have taken on as central to their visions the ideas of relativism, reality as created, and so on. For example, Donoso at one point writes, "the goal was not to trap the children inside this reality he was inventing, but rather, when they returned, the Venturas themselves. A more ticklish job, to be sure. But since after all it is the laws that create reality, and not the other way around--and since whoever wields power creates the laws--it was simply a matter of preserving authority"(p. 233). The struggle of objectiv-

ism versus relativism(to borrow the title of Bernstein's 1983 book) is one of the more important, interesting, and wide-spread challenges facing our intellectual life.

More specifically, since this debate involves "some of the most perplexing questions concerning human beings: what we are, what we can know, what norms ought to bind us, what are the grounds for hope"(Bernstein, 1983, p. 4), the issues are directly relevant to the practice of inquiry. The answers to these questions are crucial to, or determine, the interpretation researchers give to the nature of inquiry and its results. For example, if the "natural" impulse in favor of correspondence is an unfulfillable one(Putnam, 1981, p. 74), what then does it mean to say that "research has shown. . ." or that "the results of research tell us. . ."? Unless researchers take a position in regard to the issues in the debate, it is difficult to see what sense they make, or for that matter what sense others can or should make, of these claims. And, of course, if it is unclear what is meant here, then it is unclear why anyone should pay attention to researchers and their findings. As was previously noted, maybe if social and educational research had attained those "self-evident" accomplishments common to the natural sciences, this would not be a problem of interest. In the absence of such accomplishments, these questions are very important and must be addressed by the research community. To "close down the conversation" is the wrong move at the wrong time.

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